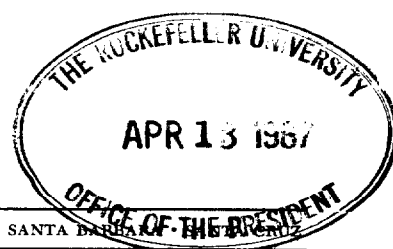


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8 April 1987

DEPARTMENT OF BIOLOGY
RIVERSIDE, CALIFORNIA 92521

Dear Josh,

Lester Barth was a student of Charles M. Child at the University of Chicago. Thereafter he spent a year, I believe, in Germany--probably in Hans Spemann's lab but I am not sure on that one (old editions of American Men of Science should have the info.)

Arthur Pollister took his degree with Robert H. Bowen who had received his Ph.D. degree from Columbia in 1920 and stayed on the faculty until his early death in 1929 (then Franz Schrader was called from Bryn Mawr--following the path of Wilson & Morgan). Although Bowen was Pollister's prof, surely E. B. Wilson must have exerted a profound effect.

I have not heard from Pollister for several years but have just been told of a recent address: Box 456, Port Morris, NJ 08349.

You will have a problem with Francis' tree. I will tell you what happened and you can decide how it might be represented.

Francis was a year (?) behind me in graduate school (although we took the qualifying exams at the same time). I was working on temperature & rates of development in frog eggs and he became interested in one aspect of the work, which he developed for his Ph.D. thesis. Both of us were being "supervised" by Lester Barth but that wonderful person left us alone but always supported us if there was need to do so. Very shortly before Francis was to finish his work (maybe Betty Ryan can give better details), Francis decided to switch to having Pollister as his major professor. Barth was somewhat puzzled and very unhappy. I was most surprised and, upon asking FJR "Why?", was told that he felt sorry for AWP because he did not have any students. There must have been more to this affair than that but FJR insisted that was it.

As you so rightly point out, it was Beadle & Tatum who really set FJR on his line of research. FJR did not go to Stanford to work on Neurospora and only began to do so, as he told me, when Beadle showed him of the remarkably exact growth rates of N. in relation to temperature. So he studied that aspect and later switched to the more exciting things that N. can do.

Much enjoyed reading the two articles. Some day you should write the true story of your attempting to crash the CSH meeting and present your results. One of the main reasons, poignant

though practical, that FJR thought you should go to Yale and work with Tatum had to do with society, not science. There was greater intolerance in the academic profession then than today and FJR really thought that a young Jewish scientist would have better opportunities graduating from Yale than from the Zoology Department of Columbia, which in the minds of many was far too liberal in the "sorts of people" it graduated. Francis was enormously fond of you and he would surely have liked to see such an important breakthrough as your's coming from his lab. He was right in giving that advice. As we two know, he was great in many ways. Is there anyone else in science today who know this? I hope so.

It is good to reflect that, in this often sad world, some things do improve--tolerance is one of them (at least in some nations and for some things).

I still think of you as in that photograph of 1945.

A good example of a postmature discovery, that also meets your three attributes, is pointed up by that wonderful remark of Thomas Henry Huxley who, when learning of Darwin's hypothesis of natural selection, said "How extremely stupid not to have thought of that!"

Science as a Way of Knowing continues to flourish but the workload has become awesome.

My very best,



*Are you familiar with "The Department of
Zoology of Columbia University 1892-1942" by
Henry E. Crompton? Lots of history.*